

On the bus from Cinci to Homerville - John Ford was on the back seat entertaining a group with coin stories. I was in the seat near the back and heard a 2 hour diatribe of expences.

He said Jim H. Murrell was ~~John~~ his
Client (of Dallas)
wealthy and sold him things

He said that he sold things to Stock for Kossiff and that Stock was supposed to add on 10% and he asked ~~Kossiff~~ to see the invoices and that they never would show them to him. He ~~described~~ helping Kossoff with information about the Killy inventory and appraisal and he asked Kossoff to show him the invoices and Kossoff died & he never saw them. Kossoff promised to do it. He cut up Kossoff and said Kossoff's bad books showed unfair tactics. He said Kaplan was a tyrant and knew nothing about coins.

Elgin Gates - bad pieces made in Hong Kong

Tabled about \$10 of formal gold
\$10 of formal gold
counterfeits which
Stonemiller obtained
and then charged
his mind.

He talked about the
Justice lectures
and their helping
Kossoff begin late
Kossoff buying
the other holdings
then he bought things
from Kossoff and never
paid him and never
could get them from him

1962 age 40 Jewelry dealer in LA.
Ford ~~sold him~~ \$3000 for 384 fine US Assay \$20

then learned he sold it to someone else for
\$2400. After checking the piece sold by Gates
found it was a cast made in Hong Kong by order
of Gates. Gates disappeared after
selling many Hong Kong made casts
of 1796 Yr old, 1794 dollars ~~the~~ pioneer
gold etc. Leo Young bought a number
of them and Harry Young still has them

We talked about
western Mill
and picking up
boxes of stuff
and buying them
for nothing

He said Ryde
coins were sold
by Ryde estate
in 1945 to Wayne
Raymond and that
Raymond sold
them. He said
Ryde died in 1926 and
Raymond appraised them
and Raymond left
them in 1945 at the
some price.

Leslie Elam, Director Marie Martin, Editor Eric Newman
American Numismatic Society American Numismatic Society Cecil Ave
Broadway at 156th st Broadway at 156th st
New York City 10032 New York City 10032 St Louis, Mo.

10 July 1997

Dear Folks,

Eric: it will be news to you that I have withdrawn the gold bar article. Leslie: I owe you an apology for snapping at you yesterday, but I must say I was sorely vexed.

Background: I have written an article that never should have been written by me. I'm not an Americanist, and I have lots to do otherwise. But I busted my butt to do a full and scholarly job, because the problem is enormous and important, and nobody else would tackle it. Yes I mean you, Eric. I have been gratified that you were willing to accept the article for publication in the AJN.

But I will not go on with it under your present restrictions. After the article was ready for the press, Bowers' book appeared, including a chapter vital for this whole investigation since it bore directly on my argument that the bars had no history previous to the 1950s. Bowers (i.e. Ford) now claims no fewer than 8 hoards of them. I responded to that serious claim in detail, with Part II of the article, and had to respond, since it pulls the rug out from under a central part of my argument. The supposed hoards are Ford's fantasies.

Now I find that you refuse to publish that part of the article. In fact I am not to be allowed to discuss this new evidence at all. Marie will allow me only to footnote Bowers' book, and to say (falsely) that I will be looking into his text later. This is nonsense: you leave me saying "My position is that the bars have no history. The most recent evidence actually contradicts this position, but I won't discuss that here -- maybe later and elsewhere." This is nonsense: no self-respecting scholar could possibly accept such a restriction. Every editor I have ever worked with would reject the article if I *didn't* include and examine Bowers' evidence. Marie knows well that the version of the article which she wishes to print is out of date even before it is typeset.

I take it the three of you have agreed on this already . At least on Tuesday Leslie was already saying that the Bowers' material is a different matter -- which it certainly isn't: it bears directly on a major point in my argument. Eric's letter was to hand recommending that Part II not be published; and Marie told me she hadn't even read it. Not much room for discussion left there, and in my judgement you need discussion badly, because I think you guys have lost your marbles.

Eric: you sent me a copy of Bowers' book to alert me to this new addition to the bar saga (and Marie had alerted me earlier). Thanks for the book. Then when I integrate a

response to Bowers' stuff into the article, you recommend that it not be published. What do you want, anyway? You say in your letter to the Society that Part II wasn't part of the original lecture anyway. Of course it wasn't: Bowers' book hadn't been published. But now it has been published, and it's clear from the text of ch. 12, on the bars, that it is at least in part specifically aimed at confuting the lecture (e.g. the ref. to Mehl). And it deliberately and specifically engages the question of the bars' survival through hoards. You expect me to ignore this in my study? Or worse, to admit to its existence but not discuss it. I would appear a right fool.

It's worse than that. If the article were to appear as Marie suggests, Stack and Ford and Bowers would love that evasion of Bowers' book: "Buttrey claims that the bars have no history; Bowers' study shows otherwise. Why does Buttrey refuse to face our facts?" I am not about to set myself up as such a target for them. They could get all kinds of mileage out of it.

Marie says, "Oh well, if Bowers/Ford/Stack respond on this point, you can just write another one on question of the hoards, which however I won't publish in AJN." Wonderful: I give them the opportunity to keep up the falsehoods, and have to clean up after them yet one more time -- and publish it where? in Coin World?

I think you guys need to turn on your brains. For good or bad, it is the fact that Bowers' book makes a serious assertion which goes right to the heart of my study and blows a hole in it. And you ask me effectively to ignore it. I cannot possibly leave a hole like that in my argument. As scholarship it is ludicrous. Nor am I about to expose myself as a sitting target because I left the argument open to serious question.

If you object to the style or the tone or the length or the structure of Part II, these things can be altered. Already ~~in~~ Wednesday, in the presence of Marie, I cut it drastically from 12 pages to about 4. This also eliminated all the Brother Jonathan stuff, and all the serial account of what Ford has been up to. But the analysis of Bowers' ch. 12 has to stay, because without that the whole paper is in danger. I don't mind if it is published as an "Addendum" rather than "Part II", or whatever you want to call it. But a simple footnote reference to Bowers? -- No. It would compromise me as a scholar, and compromise the study itself. If you accept the article on these terms, fine; if you reject it, fine. But if you tell me that I am not permitted to examine and discuss the most recent, and possibly most damaging evidence, then forget it. I can only wonder what is up with the AJN.

Yrs,

Tea

The 4-page version

ERIC P. NEWMAN NUMISMATIC EDUCATION SOCIETY

6450 Cecil Avenue, St. Louis, Missouri 63125

Marie H. Martin, Editor
American Numismatic Society
Broadway at 155th St.
New York, NY 10032

October 27, 1997

Dear Marie:

Thank you for the revised page proofs of the Buttrey article sent October 22, 1997. As I indicated previously the presentation is very forceful and shows an immense amount of research on a complex matter. Many expressions have been clarified in the editing.

Every author and editor has different styles and a few further suggestions from here may be welcome. Some of our suggestions have been submitted previously. Further explanations may be discussed if you wish.

p. 89 I suggest adding at the end "plus the author's footnote Addendum". This is to show that Buttrey wrote comment it and that it was added.
line 2

p. 90 The 1852 date is not quite accurate as has been pointed out before. The first publication mentioning new line 4 private gold was in 1850, revised in 1851 and again in 1852, all by Eckfeldt and DuBois.

The words "struck in 1851" is not accurate enough and should be "struck in prior years".

Banker's Magazine also published some material which was copied by Eckfeldt and DuBois.

p. 91 In the description of the chart the word "COINS" should be "ISSUES" as Moffat and Kohler are mentioned because they produced bars.

p. 101 "did not offer a single western American bar" should line 4 have added to it "considered here".

p. 102 In "Not only do our western bars" the word "our" gives line 12 a wrong impression. The words "considered here" should be added after the word "bars". The word "our" should be changed to "the" because the word "our" might be construed to involve ANS ownership or sponsorship.

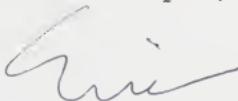
p. 105 "much bigger bars than exist today" is a speculation. The content of the Central America include very large ones. I suggest "big bars as found on the Central America" in place of the above.

p. 106
footnote 30.
last line of
2nd paragraph. "recent" should be added in front of the word "western".

footnote 30
third paragraph
line 5 "recent" should be added in front of the word "western".

p. 107
footnote
line 2 insert "small" in front of "bars".

Hoping we have been helpful,

A handwritten signature in cursive script, appearing to read "Eric P. Newman".

Eric P. Newman, President

2 Pages

Marie Martin: 11/5/97 noon FAX to 212 283 4680

Please add these items to my suggestions in my letter of 11/1/97 as I did not make some of the items clear enough or accurate enough to satisfy me. Please have others check me.

(p. 2) changes to p. 95 of the article should be changes to p. 93 -

(p. 2) p. 96 lines 3, insert after Moffat " Hentsch & Berthon,

Kellogg & Hewston, and Wiegand

(p. 2) p. 96 line 6, strike "at the end" and substitute "considered later"

(p. 3) p. 99 2nd P line 13 . . . In substituting for "them" use the words "of the bars first appearing in the 1950's" instead of "of the bars appearing in the 1950's" as I previously thought.

(p. 3) p. 101 this reference is to line 5. We could add a further phrase "but only two in an unpublished list"

(p. 3) p. 102 substitute "other than the exceptions noted" line 2 instead of what I previously suggested.

(p. 4) p. 104, footnote 25, line 6 strike out "Stack's"

(p. 4) p. 104 footnote 25, line 2: strike out ", and Harvey Stack, John J. Ford, Jr., and New Netherlands Corn Co.; eng in" - Insert before NASCA "See"

(p. 4) p. 104 correct my suggestion as follows:

Second P line 2 Insert after "bars" the words "newly appearing in the 1950's and 1960's."

(p. 5) p. 108 lines 2 and 3. Strike out "purchased from Stack's"

If I have other suggestions I will make them.
Eric

P.S. Elder (1929) lot 975 is the same as Mehl (1933) item 2660.

Marie Martin 1PM 11/5/97

Page 2

On page 2 section 2 of the Budney letter to you dated 10/28/97 there is a list of six sources of western gold bars in the Lilly collection. I have tried to remove the source of all bars to the Lilly collection to avoid having to explain the six named sources in the suggestions.

I do want to explain to you and ANS that the six named were all customers of Ford or New Netherlands or business associates. They apparently acquired their bars which they sold directly or indirectly to Lilly from Ford or from the same source as Ford. Keefer was a collector in Chicago, specializing in pioneer gold; Hallenbeck is the Kagan firm, in which Don Kagan was a young pioneer gold specialist, but the older Kagens had a great interest in it and bought the Clifford collection I believe. James, Inc (Karp) was a dealer in Louisville. A. Kosoff was a dealer in Los Angeles, W. Amelingmeier was a book (numismatic) dealer living close to Ford and a close friend; and Smith & Sons were in Chicago and pioneer gold specialists. Keefer and Kosoff are deceased and the Smith & Sons owner is also. I have not heard anything about Amelingmeier for a decade or more as far as I can tell of James, Inc so I don't know whether they are living. The Kagans are very much alive. I will discuss this if you wish.

Eric

Marie Martin FAX 212 283 4680

2³⁰ PM 11/5/97

Please correct a transposition which happened
due to haste:

p. 99 footnote 19
Eliminate old footnote suggestion
Change footnote to

Exceptions are: C. Wiegand mixed metal
issues, S. H. Chapman, 21 June 1909 (Jewett),
986, 987; Kellogg - Hewston issue, Elder,
13 December 1929 (Steindhardt), 975; C. Wiegand
issue, Mehl unpublished, 1933 (Newcomer), 2750;
and Hentsch & Berntson issues, Stack's
Numismatic Review 101, June 1943, pp. 7-8.

11/5/97 2³⁰PM FAX 212 283 4680

ANS Suggestions for letter to Grodd
I. Budney 10/28/97 letter whenever
reply is advisable.

ANS as you know undertakes to publish matters
of numismatic scholarship, ~~this is often~~
written by others.

Such writings often result in differences
of opinion and ANS avoids taking a
position on such matters particularly as to
authentication.

ANS should thank ~~Mr~~ Budney for furnishing
~~the~~ the exhibits to the letter and was not aware
of some of the facts pointed out. ANS did
not have in its library a copy of the Waldo
Newcomer pioneer gold ^{reportable} lost by Mehl and
would appreciate a full copy for their ^{ANS} library.

ANS is glad to call ^{your} attention to the Elder ^{date Dec 13, 1929,}
lot 975 in which the bar mentioned in your
Mehl lost # 2660 is the identical item as well
as the Nummatist Mar 1930 p. 174, 176 (identical item).

ANS was glad to see a conciliatory attitude
shown in your letter and has attempted to edit
the text into a form which will avoid inaccuracy
and unfairness as well as stimulate debate on a
complex subject.

ANS invites the submission of other points
of view presented in a scholarly manner and
in conformity with ~~ANS~~ ANS standards,
including editing.

Prof. T.V. Buttrey
6 De Freville Ave.

Cambridge CB4 1HR
England

tel/fax 011-44-1223-351156
e-mail tvb1@cam.ac.uk

22 September 2000

On Scholarly Integrity at the American Numismatic Society

While we are all aware of the terrible difficulties which beset the ANS, both the financial situation and the way it has been handled, another problem has been developing of which most of the Fellows are probably unaware.

We have been assured by the officers of the Society that its (and their) commitment to scholarly activity and scholarly integrity will not be compromised. The following is an account of how in one instance that commitment has actually been met. I ask your patience: it is a long story, but it is a serious one, and I prefer to make the case in detail. I intend that it be published later in a fuller form, but want to get it out to all of you now in draft.

Permit me too to recall on my own behalf that I have served the Society in many ways for almost 50 years -- not just as a Fellow, but as an editor and sub-editor of numerous MSS submitted for publication, creator of hundreds of abstracts for *Num. Lit.*, contributor to both *Museum Notes* and *NNM*, curator *pro tem.* of the Roman vault, donor to both the collections and the library, member of Council for 21 years, active Chairman of the Publications Committee, Benefactor, Huntington Medallist, and Corresponding Member of the Society. What follows I do not write lightly.

The Background:

The problem is the authenticity of certain gold ingots. If they are genuine they are of real numismatic and historical interest; if not, they are a fictitious imposition not just on collectors (they are expensive) but on our studies.

For several decades John Ford and Stack's have been putting on the market ingots purporting to be (1) Spanish colonial products of the mint of México in the 1740s, and (2) 19th-century Western American gold bars, produced by the San Francisco Mint or more frequently by private Western mines, assayers, or mints. There is also (3) a piece alleged to have been produced in Tubac, Arizona in 1707.

The Mexican bars number perhaps 60 or 70; the American bars in the hundreds. They have sold, and are being sold, for prices in the \$1000s or \$tens-of-1000s; the most expensive of which I have a record was sold by Ford for about \$40,000. Their gold content is high, which makes them attractive to anyone who loves to accumulate gold. But what makes them valuable -- and important

to us -- well beyond their bullion content is the historical context woven about them with apparently authentic detail. Thus the Mexican bars are claimed to represent the King's Fifth, and to have been recovered from an 18th-century Spanish treasure ship. A purported San Francisco Mint bar offered by Stack's in their auction of 11 June 1997 was said to have come from a famous Pacific Ocean shipwreck. Such stories raise the value of the object very considerably (see below), while engaging our scholarly interest.

There are two issues here: whether the bars are genuine, and if they are, whether the stories are genuine.

The Mexican bars (1) are dated in the 1740s and bear the México mintmark. They all belong to one family and stand or fall together; there are no other ingots from the Mexico mint for comparison. I gave a paper on them at the International Numismatic Congress in New York, 1973, showing why they should be taken to be 20th-century confections, and why the historical claims in which they had been embedded did not stand up to scrutiny.

This could have been the stimulus for further discussion, and submission of such counter-evidence as there might be in support of the bars' authenticity. Instead, Ford, who put this material on the market -- that he did is secure -- threatened a libel action against me personally, and against anyone in the American numismatic press, including the ANS, who might dare to publish it. The ANS declined to publish it so it appeared abroad (Buttrey 1973), and Ford and Stack's continued to put these (and the US bars) on the market. They appeared in auction catalogues accompanied by fictitious texts intended to establish an 18th-century context for them. Meanwhile my published condemnation of the bars was never answered by them.

* I emphasize that I was not the first to conclude that the Mexican bars were fakes. When first I ran across them I discussed them with two well-known New York dealers, Henry Christensen and Henry Grunthal, both of whom condemned them; and later found that other dealers both in the US and abroad held the same opinion. But the trade has never policed itself, and no-one was willing to tangle with Ford or Stack's. Nor had anyone actually sat down to make the case for or against the Mexican bars. So I decided to do so myself, partly out of a personal interest in Mexican numismatics, partly because I objected to corruption of the historical record, any historical record. On the evidence, both internal and external, I concluded that the Mexican bars were false, and that the stories surrounding them were historical fictions concocted by the dealers. In fact the ANS Council published a warning against them ('Policy Statement', in ANS Newsletter (Winter 1984), p.5).

* I must also emphasize that I do not have, and have not had, any financial interest in any of the ingots discussed herein. I do not and never did buy them, sell them, or collect them, nor act as advisor to anyone who did or does. My interest is scholarly.

A subsequent note on the Mexican bars (Buttrey 1979) was published in the *Bulletin on Counterfeits*, an organ of the International Association of Professional Numismatists (i.e. dealers), under the editor, E.G.V. Newman, not a dealer but an eminent professional assayer, who had independently concluded that the bars were false, and who later published his own refutation of them (Newman 1990). Again there was no published response to my note from the purveyors of the bars, although letters were circulated privately attacking the note and myself (Ford: Buttrey is paranoid, Buttrey is the agent of those who are out to destroy me, etc.).

Later I produced an article on the Tubac ingot (3), another of Ford's products, showing it too to be false (Buttrey 1981). What was particularly disturbing about this piece was that it had been given historical credibility by being published in the 1964 edition of the *Encyclopaedia Britannica* on a plate of illustrations provided by Ford for the article "Numismatics", and accompanied by other pieces stemming from him which I have argued are also fakes. -- Again, there was no response from him. In fact doubts about the authenticity of the Tubac ingot had already arisen in Arizona, independently of my own study. A request for clarification was directed to Ford through the EB but no response was ever received from him.

Items (1) and (3) were just the tip of the iceberg. The mass of questionable bars falls into group (2), the purported Western American bars. These are not just oblong hunks of gold, but are characterized by reasonably careful shaping; a high gold content (usually), ranging in the 800s or 900s parts per 1000 fine; and certain information stamped into the bar: the fineness, the name of its purported assayer (or mine, etc.), often the silver alloy content, the weight, the value in US dollars, a serial number, and often a date. That is, these are not crude lumps, but specific individual items of high intrinsic value, apparently guaranteed by the information stamped upon them. (They are also very easy to make. Characteristically the bars are created simply by hammering individual letter and number punches into the surface of a cast ingot, although some bear more complicated logos.)

Although originally the Western American bars had not been my particular concern I collected such information as came to hand, and ultimately concluded that while there is some evidence for such bars earlier, those put on the market by Ford and Stack's were almost entirely false.

* Here too I am by no means the only person, or even the earliest person, to doubt the Western American bars. There has been distrust of them among some students and dealers for years, but as with the Mexican bars there has been little publicly admitted disapproval of them.

In March 1996 I gave the Huntington Lecture at the ANS, on the subject "False Western American Gold Bars", making the case against the authenticity of the Ford-Stack's bars. The published form of the lecture appeared in the AJN the next year (Buttrey

1997), not without some soul-searching by the then Executive Director of the ANS, Leslie Elam: Stack had already threatened a law suit after the lecture.

It should be stressed that neither Ford nor Stack's have ever suggested, much less conducted, a round-table discussion of their material, where all views might be considered. They maintain that the bars have been insufficiently studied by those who doubt them, yet publish almost nothing on them themselves beyond their auction-catalogue blurbs; and they are unwilling to make the bars available to others for examination (see for example [5] below). As early as 1979 they had been urged by E.G.V. Newman to provide several of the Mexican bars for examination, but never did so (Newman 1979). Their technique has been to stone-wall, to attack others' discussions and reputations, and to threaten reprisals against others' publications.

That is the Background, all of it publicly known, or knowable. What follows is an account of how, after my 1996 lecture and its publication, the officers of the ANS accommodated to the pressure brought upon them, by these and other dealers, culminating in the publication of the article by Michael Hodder in the latest number of the *American Journal of Numismatics* (Hodder 1999 [2000]).

The Account:

In this discussion 16 paragraphs are numbered, [1] to [16], to mark the points raised particularly regarding the behaviour of the officers of the Society.

In reaction to Buttrey 1997, Ford and Stack's insisted on the right of reply. This was the first time in about 30 years that they were willing to go public. Their agent was Michael Hodder, an author of various numismatic articles, who has been their paid consultant for a good many years. That is public: he is carried as Consultant on Stack's list of the staff of the firm.

[1] Hodder submitted to the ANS an extensive MS of over 100pp, uncharacteristically long, purporting to be an academically objective affirmation of the authenticity of the Ford-Stack's ingots. The article was accepted for publication in spite of Hodder's position as the principals' paid apologist; in spite of their (and his) known financial interest in the bars; in spite of their known association with false bars and concocted history; in spite of the fact that Stack's had continued to sell the Mexican bars after the specific public warning against them by the ANS Council.

Hodder's principals also wanted him to give a lecture at the Society, to answer mine. This was agreed to by the Society, and was delivered at the Society in April, 1999, essentially a précis of the then-unpublished MS.

[2] Kroll introduced Hodder's 1999 lecture as if the subject was one of purely academic debate. Again, there was no hint of

Hodder's personal commercial stake in this material; nothing about his employment by and on behalf of Ford and Stack's, whose corruption of our studies I have documented above. As to the position of the Society, Kroll stated

Proponents on all sides must have access to an open forum. This is the philosophy of the ANS Committee on Publications and, I think it is fair to say, of the Society in general. Not to take sides, but to provide a forum for serious, informed debate. May the best research and arguments win -- on their own merits.

In the first place this is simply false: the "open forum" is on this issue now specifically closed (see below at [9] and [16]). As to the ANS, there is nothing here about scholarly commitment or even scholarly decency; the position now is "not to take sides", even in a case where the very material of our studies is at risk from the unprincipled.

Kroll's statement is not a one-off. He wrote to me in a similar vein, "we only supply a forum". That is how the officers of today's ANS see its role and theirs, which cannot jibe with their claim to their maintaining the Society's scholarly integrity. The effect of their current policy is the abandonment of the Society's role as a leader in serious numismatic study. Their behaviour in maintaining their new agenda has been, in my view, not just unserious but unethical. This is a weighty charge; for the documentation, see what follows.

[3] To the lecture itself: Hodder stipulated that the ANS disallow the recording of it, to which the Society agreed, although my own had been taped with ANS consent by Harvey Stack and two lawyers. This is a minor point, but indicative of the ease with which the Society now permits itself to be pushed around.

Hodder's lecture cannot be recovered. But John Kleberg reports that he raised a problem in the question period following it. Hodder and Ford had contributed a chapter to Bowers 1997, describing discoveries of several hoards of the western gold bars, hoards which of themselves would support the authenticity of the bars. These discoveries are completely unknown aside from Hodder's and Ford's assertions (except for one, the so-called Franklin hoard, which is well-known but was not a hoard at all and whose contents included false material (Martin 1994)). In the discussion afterwards Kleberg pointed out that in one instance the account simply could not be correct; to which Hodder replied to the effect that, "Well, it's only intended for the boobs [sc. their customers]", which illustrates something of the flavor of these people.

[4] It is reported that neither Kroll nor any of the other ANS officers present objected to, or even commented on this appalling admission; nor apparently did it move them to consider the ethical quality of either the lecture or the to-be-published MS from which it derived.

[5] To illustrate his lecture Hodder provided several bars which he himself set up in an exhibition case, and himself took down from it. He refused to allow John Kleeberg, the Society's own Curator of Modern Coins and a distinguished scholar, so much as to touch the bars. Why did the Society permit this imposition?

Now as to Hodder's MS: I provided a reader's report urging its rejection: it is weak in research and in argumentation, it is prolix in discussion of irrelevant detail (e.g., how the mints used to make bars, any bars), and it does not in fact demonstrate or even support with credible evidence the conclusion which he asserts, that the Ford-Stack's bars are genuine.

[6] I was told by the then ANS editor, Marie Martin, that it had already been decided to publish Hodder's article regardless of its quality or of any reader's report. This was explained as being partly out of a desire by the Society not to appear to take sides in the discussion of the bars, but also as a kind of trap: since Ford et al. have published very little on the bars Hodder's article would bring their arguments into the open and provide the material for a precise rebuttal of their claims.

It is a poor idea, and self-destructive editorial policy, to publish something apparently serious in your scholarly journal which you know is unworthy. The real reason, I have no doubt, was simply to get Ford and Stack's off their back.

But the MS has its importance: in it Hodder allowed three significant points:

a. He admitted that Ford's Tubac ingot (3) was false. This is the first-ever admission on their part that Ford had handled fake bars. (p.94 fn.21)

b. He havered over the authenticity of the Mexican bars (1) -- "if these bars are real" (p.143) -- and suggested that even if 18th-century in origin they might not have been produced at the México mint after all -- again the first admission that something might be wrong with the Mexican bars.

c. Which introduces the most astonishing admission: Hodder allowed that Ford had misrepresented the source of the Mexican bars:

At the time of their appearance on the market, these bars had acquired what may politely be called a legend, that attempted to explain how they were discovered (fn).

fn: The legend can be read in Buttrey (1974, 28-29). It takes the form of an undated private communication from John J. Ford, Jr., to Buttrey, in which a fanciful tale of a secret treasure map found in an old archive, a 1748 Spanish shipwreck, and a few other elements are woven together into a tapestry of wonders. The letter refers to events dated 1951 and 1955-56 and gives the latter range as the earliest acquisition dates for these Mexican bars. Coming as it did before such sunken treasure discoveries as the *Nuestra Señora*

de Atocha and the *S.S. Central America*, the legend may have had crude attraction in the 1960's. To a modern reader, it can be read as a marketing tactic designed to give the bars the flavor of recovered treasure. Unfortunately for the bars' subsequent numismatic history, the legend became the starting point for the only focused study yet published on them. (p.90)

Also later,

The story that the Mexican bars were shipwrecked gold made at the Mexico City Mint is an obvious legend meant to enhance the marketability of the bars. (p.143)

"Had acquired a legend" is disingenuous: the Mexican bars came from Ford, and the "legend" was Ford's invention, "meant to enhance the marketability of the bars". As to, "Unfortunately for the bars' subsequent numismatic history...", these were Ford's misrepresentations and could have been retracted by him at any time. On the contrary, the bars' fanciful pseudo-history continued to appear in the auction sales of the bars to encourage their purchase. The 1984 ANS Council policy statement in warning, mentioned above, was simply ignored: as recently as Stack's' sale of 2 December 1997 one of the Mexican bars appeared embellished by the usual fictions --

...reportedly found on or near Florida's east coast in the mid-1950's... Some researchers theorize... King's Fifth...

Hodder neglects to observe that misrepresentation for the purpose of raising the value of what you are selling is, in law, fraud, and is actionable in both civil and criminal proceedings.

Why these admissions? Presumably a. and b. arise from the impossibility of justifying any longer the Mexican bars and the Tubac ingot in the face of the evidence which I and others have assembled. As to c., it is very curious. The admission is couched in a jocular tone, as if to say, Well, this is just marketing, the way everybody does it.

Now it may be worthwhile to publish such extraordinary admissions, but they raise a larger problem with Hodder's MS. Hodder has not only acted as Ford's agent for a good many years; Ford has been an important source of information for him, if not indeed of inspiration:

John Ford, whose name appears in places in the text to follow, was more than generous with his time, expertise, and collection. There is no one alive, today, who knows more about western Americana than he does, and I am proud to call him my friend. [MS p.90; deleted from the final publication]

Ford had fraudulently misrepresented his own material -- that was admitted. How then could anything be believed in Hodder's MS, to which Ford contributed so importantly, and whose very purpose was to claim as genuine Ford's numerous Western American bars?

My own position therefore was that the ANS should not touch this article, not just because of its poor quality but more importantly because it is tainted.

The prolixity of Hodder's MS is another point. It is in fact not characteristic of his other published work, but it is of Ford's, which has appeared mostly in auction catalogue entries of enormous detail, purporting to authenticate the bar or bars on offer. Dr. Wartenberg read the MS and said to me that she did not believe Hodder to be its sole author (i.e. it was written by both Hodder and Ford). I think that is correct. (The reason why the real authorship is obscured is perhaps to be explained by the rejection of such a designation a few years back by Dr. Metcalf: Hodder had delivered a paper, which subsequently was offered to the Society for publication as a Hodder-Ford contribution, and Metcalf declined to accept that authorship.)

[7] Yet even though Wartenberg believed the attribution of the present MS to Hodder alone to be false, she permitted publication under his sole name. I submit that such knowing misrepresentation in a scholarly journal is reprehensible.

In his MS Hodder claimed to make a new and important contribution to the whole question of the dating of the Ford-Stack's Western American bars -- in his terminology, "western assay bars". My contention has been that virtually all of the Ford-Stack's bars were unknown before the 1950s; Hodder argued to the contrary, that the evidence of the San Francisco Mint Archives showed that such bars were already around in quantity in the 1850s, and that therefore my claim that they appeared suddenly beginning only in the mid-20th century (and therefore are false) cannot be sustained.

At this point, an excursus is necessary. In California in the Gold Rush days the metal discovered was brought in many forms to private assayers or minters, and from 1854 to the newly-founded Branch Mint of the US. The gold arrived in many crude forms, as dust, rocks, amalgams, unrefined or basically refined lumps or bars, etc., to be reduced and refined. The customer could simply sell the gold for what he could get at the smelter, i.e. take its value in coin, or get back the refined gold itself if he thought he could do better with it elsewhere.

There is no doubt that some firms produced hundreds if not thousands of gold bars with the various assayers' markings as noted above. Some have recently been found in the wreck of the Central America which went down in the Atlantic in 1857, and are of course absolutely genuine. However these differ from Ford's and Stack's bars in being mostly very large (the largest weighs 80 pounds), since they were intended to transport value, not to be used as money. This is why they have not survived otherwise -- representing too great a value and being in any case too awkward to use other than as a means of conveying bullion. Contrarily, most of the Ford-Stack's bars are so small that many of them could have circulated as money, and could easily have survived. So the

question arises, When their relatively small physical size and small value would make them easy to preserve, why did these numerous pieces not survive in earlier collections, or in earlier records?

In his MS Hodder claimed to have found just such records in the San Francisco Mint Archives of the 1850s, which include references to this very sort of "assay bars" in the registers which acknowledge customers' gold deposits.

Ford too made a claim about the Mint records: for this, a second excursus:

Over the years Ford put on the market bars purporting to be products of the San Francisco Mint itself: they bear its name, the date 1865, and the other kinds of information on fineness etc. already noted above. My own unpublished study concludes that these too are false, but at this moment I have a different point to make. On their first appearance in a 1974 auction Ford stated that he had visited the Archives in 1971 and seen the register in which their production was noted, so that he could date them to early in the year 1865. This is important, because their interest lies not just in their rarity -- no other bars of the Mint from this period are known to survive -- but because (said Ford) they came from the wreck of the ship *Brother Jonathan* which famously sank in a storm off the California coast in July 1865.

This story of course enhanced their market value, and it became more elaborate over time. By the time one of the bars was sold by Stack's in their 1997 sale the allegation had become fact, and Ford had seen in the Archives not only the register of their production but their receipted delivery to a Mrs Keenan who went on to be a passenger on the *Brother Jonathan* and to perish in the wreck. To excite our interest further it was discovered that Mrs Keenan had been a prominent San Francisco madam, travelling with six of her employees to Seattle to enlarge her business. That the present value of the bar rises with each one of these fascinating associations -- bar + U S Branch Mint + Mrs Keenan + madam + *Brother Jonathan* + famous wreck -- is clear from the results of the auction. Stack's' bar was worth roughly \$6000 in fine gold; it sold for \$33,000. That is, the rich and interesting story alone was worth more than x4 the gold.

Mrs Keenan was a real person, and a real victim of the *Brother Jonathan* wreck. But we have only Ford's word for her association with the bar, for the bar's association with the wreck, and indeed for the record of the bar's producion at the Mint; and Ford is known (on Hodder's admission) to have made false historical claims in the past, "to enhance the marketability of the bars".

Thus Hodder and Ford both used the San Francisco Mint Archives, which purportedly provided evidence for the claims of each -- Hodder, in the references which he said he discovered to his

"western assay bars"; and Ford, in the evidence which he said linked his bars to the Mint and to Mrs Keenan, etc. These claims needed to be verified, but the Mint archives have not been published: the only way to check them is to go to San Francisco yourself. I did, in April 1999, and spent a week going through the records for this period.

To be brief, Ford's claim is a complete fabrication. The daily signature book for visitors in 1865 survives. It has been checked three times -- by me, and twice independently by two members of the Archives staff: there is no Mrs Keenan. The registers for this period have been preserved: none indicates either her deposit of bullion, or her receipt of bars in exchange. Nor is there even any register which refers to the production of these bars. All of this has been concocted by Ford; and there is independent evidence that the bars themselves are false.

[8] I ask your indulgence for going through this matter in such detail, but there is a point: these falsehoods of Ford's were known to the officers of the ANS -- I first discussed them with Kroll when we met last year -- but they fell on unwilling ears. I maintained that on the basis of these data alone Hodder's article should have been rejected, since he too had gone through the Archives and would have known perfectly well that Ford had falsified the archival evidence. Yet he kept that information quiet -- a nice instance of scholarly betrayal -- and has gone on working with and for Ford, authenticating him and his works to the public.

Ford aside, there is also the question of Hodder's own scholarly claim. In his MS, in the text of his discussion of the evidence of the Mint Archives for the 1850s, he repeatedly used the phrase "western assay bars", by which he means the specifically marked bars described above (as against any crude and uninscribed ingot), characteristically the Ford-Stack's bars, as if their presence in the records was easily established. In fact this is Hodder's invention: the phrase "assay bar" occurs very rarely in the Mint registers. In his MS Hodder gave four specific examples of them (pp. 36-37), e.g.

January 9, 1855	Wells, Fargo & Co. deposits for B.
Levy 23.74 ozs. of Assayed Bars	
November 19, 1855 oz. Assay Bar	Frontier Deviery & Co. deposit a 20
November 19, 1855 Bar	Henry Hentsch deposits a 76.82 oz. Assay
March 6, 1856 Assayed Bar	Husth & Hunter deposit a 39.07 oz.

Nothing could be clearer, but all four examples are false: for the entry claimed for 9 January 1855 there is nothing corresponding in the registers at all; for the other three the registers clearly read "bar" tout court, i.e. any oblong object. Not having found what he wanted in the Archives Hodder simply inserted the word "assay", doctoring the record to make it appear

that such "assay bars" had been well-known at the Mint in the 1850s.

Hodder's falsification of the Mint evidence is essential to his argument. Without it he has no other evidence to show that the Ford-Stack's bars were in existence before the mid-20th century when Ford began to put them on the market. And Hodder's falsifications too were known to the ANS prior to the publication of his article (for which, see below).

[9] Moreover there is to be no opportunity for anyone now to expose within the ANS what Hodder has done. Already last summer I was informed by Kroll that not only was the Council of the Society determined to publish the article, regardless of content (i.e. the claim that the publication of Hodder's article would open the subject to public discussion); but that thereafter the subject was closed: there would be no more reference to it in any ANS publication (though that was later compromised under further pressure from the bars' supporters: see below at [14]-[16]). There is of course no alternative forum in the country to the AJN: it is the only scholarly numismatic publication that there is. When I protested to Kroll his response was, Well, you can always set up a web-site.

There is more to come. In August 1999 I gave an address at the annual meeting of the ANA on the subject of the bars. Both Arthur Houghton, the then-President of the ANS, and Dr Wartenberg were in the audience. The lecture included the report of Hodder's abuse of the Mint evidence, and demonstrated it with a transparency of his MS where he claimed to have found the entry "assay bar", set against a transparency of one of the actual register pages cited by him, showing clearly that it did not say what he claimed it said.

[10] To my mind this clear evidence of falsification imposed on the officers of the Society an unavoidable obligation to get rid of this article and this author straightaway. To the contrary, the reaction of both the President and the Executive Director, both at the meeting and after, was zero -- not a comment, not the slightest intent to deviate from their determination to publish Hodder, and thereby to gratify the dealers.

Subsequently there has been considerable correspondence among us, by both mail and e-mail. I wrote to President Houghton to remind him of the fraud, hoping that a second exposure would move him. It didn't, nor did a third. On the fourth try, when I urged the same point again with Kroll and Wartenberg too -- and sent them all hard evidence of Hodder's falsification, pleading that it was thoroughly dishonorable for the Society to be involved in this business -- I finally got a response from Houghton, urgently asking that I provide the whole of the specific evidence of Hodder's fabrications regarding "assay bars". I did so, arranging (from England) with my contact at the Archives to photocopy all the necessary register pages and send them directly on to the ANS. The result of this you will see below at [12].

[11] Meanwhile I had been in separate contact with Kroll. He had not been at the talk but I informed him of my findings about Hodder. He too was not moved to cancel publication, and made it clear why: that not to publish might cause reprisals

because of [Ford's and Stack's] control of the numismatic press (and that is what is really the decisive factor here).

There could be no clearer evidence of the Society's abdication of scholarly responsibility out of fear of the dealers. But Kroll allowed that since I had found Hodder to have falsified his sources it might be possible, once the falsifications had been published [!], to persuade Council to change its mind and permit a reply.

Now that you have discovered falsification in the Hodder paper (assuming Hodder's "improvements" in the mint record will not be excised at the last minute from the printed version)... [I suggest] that you submit a reply... the matter would get hashed out before the Council... with what result I don't know...

What can one say? Prof. Kroll is not only a Fellow of your Society and an academic -- indeed a professor in a major department of a major university -- but specifically Chairman of your Publications Committee, charged with overseeing its scholarly publications. Yet he is willing to publish an article which supports its argument by faked evidence which he knows to have been faked. I replied, "If you were the editor of an archaeological journal, and were about to publish an excavation report; and you discovered that the chief excavator had salted the dig and was now including his fraud in his report, what would you do regarding the report, and regarding him?" Kroll has never responded.

I believe that the question should now be, "If you are a Fellow of a learned society; and you discover that its leading editorial officer has authorized publication in your Journal of an article whose readers are not warned that its argument depends on faked evidence, and the officer knew that it was faked... what would you do regarding this officer?"

(A personal aside: I say "your" because I have had enough of this and have resigned from the Society.)

It gets even worse: "...assuming Hodder's 'improvements' in the mint record will not be excised at the last minute from the printed version" -- yet Kroll himself was to encourage their excision [!]. On Houghton's request I had in good faith entrusted to him and Kroll my unpublished research proving Hodder's dishonesty, evidence gathered by me personally at considerable trouble and expense, hoping that having seen it all in black and white they would agree to pull Hodder's article. Alternatively, if they did nothing (on the previously stated principle that he should be allowed to entangle himself) at least the rest of us would be able later (though of course not in any

ANS publication) to use that evidence in exposing what Hodder was up to.

[12] From their past record I anticipated that Houghton and Kroll would do nothing, so that it was a gratifying surprise to hear from them that they had changed their mind, that they agreed that the *Journal* should not publish these untruths. But had I known what they then intended to do I would never have entrusted any of my material to them. Their solution to the problem was to appropriate my evidence, and without my knowledge or consent to hand it over to Hodder, asking him to edit out the fraudulent bits [!].

I've faxed John Hedger [of the Archives] for the relevant pages from the Weight Clerk's register to verify the discrepancies between the sources and Hodder's excepts. When verified, I will be writing to Hodder requesting that he make the necessary corrections in the page proofs...

(e-mail of Kroll 10

September 1999)

My protests at this were as vigorous as they were fruitless. Hodder was permitted, even encouraged, to sanitize his text, and it was published earlier this year in the *AJN* (Hodder 1999 [2000]). The false reference to the deposit of "Assayed Bars" by Wells Fargo on 9 January 1855 was replaced by a genuine reference to Drexel Sather & Church depositing an "assay bar" -- one of the few actual such references in all of the archives -- which however was "rejected by Melter and Refiner as containing base metals -- hardly evidence for the gold bars under discussion. The doctored reference to 19 November 1855, Frontier Deviery & Co., which I had used to illustrate the misrepresentation in my lecture, also disappeared. The other two genuine references were altered to delete "Assay(ed)" (pp.116-17).

Nonetheless Hodder continued to sprinkle his text with the phrase "western assay bars", and went so far as to affirm, "The San Francisco Mint routinely received private assayer's bars for melting..." This too is an invention; in literally thousands of entries such bars are attested possibly as many as three times. It is an invention necessary for the argument that such bars as Ford's and Stack's were genuinely about in the 1850s. In the revision of his MS for publication Hodder also introduced a new falsehood. As noted above, the Mint Archives survive for 1865, documents in which Ford had claimed to have seen references to the production of his group of gold bars, and to Mrs Keenan's receipt of them. Had this been true, Mrs Keenan's name would have had to appear in at least three separate documents; in fact it does not appear anywhere in the Archives: she had nothing to do with the Mint, or the bars. But now there enters our old friend, the disappearing document, so useful in fraudulent pleading. At the Chicago meeting Hodder claimed that Ford had drawn his evidence from an archival document which was later discarded. Subsequently he inserted in his MS for publication confirmation of Ford's contention --

In 1982, many of the mint's records were destroyed. One of the documents destroyed was the Weigh Clerk's Bullion Ledger, June 1854 to March 1873.
(p.116 fn. 50)

There was indeed a clear-out in 1982, but the second sentence above is false. As I write these lines I have before me a photocopy of the disposal record, dated 10-1-82, listing and describing the documents involved, item by item. No item even remotely of that title, or of that date range, is listed in the disposal record. The material disposed of was mostly 20th-century, and nothing included was earlier than 1882. -- Again this is a falsehood which is materially significant, since it supports Ford's claim to having found evidence in the Mint Archives which in fact he did not and could not.

[13] The purpose of Hodder's article is to authenticate doubtful numismatic material to the benefit of a particular group of dealers. The text is not just a parody of scholarship; it is deliberately dishonest. It is bad enough that this piece now stands in the *Journal* as the last word on this serious historical problem, with all the respectability that appearance in the *Journal* conveys in our discipline. What is truly outrageous is that the officers of the ANS knew that it was dishonest and published it nonetheless, indeed connived at hiding its dishonesties.

Nor has this been the only opportunity offered to the dealers in the matter of the western gold ingots. The formal position had been that Hodder 1999 was to be permitted in answer to Buttry 1997, and that was the end of it as far as the Society was to be concerned. But they caved in to the importunities of another dealer, and another piece in support of the bars appeared, this time in the Fall 1999 issue of the *Newsletter*, which carried a statement by Q. David Bowers.

[14] I believe that the *Newsletter* has never before carried letters or personal statements, and certainly not from anyone in promotion of his own commercial interest. Why now? I cannot document the point, but Bowers is a very insistent individual; and I am told by a source which I believe to be credible that on Bowers' insistence an agreement was struck whereby his letter would be published, while he would donate several hundred dollars to the celebratory banquet to be held on Houghton's retirement from the Presidency. If this is true, he bought his way in.

[15] Secondly, the author is introduced by the Editor simply as "ANS Fellow Q. David Bowers". He is in fact a notable dealer, and one who, like Ford, Stack's and Hodder, has a financial interest in the western bars. As recently as last year he offered at auction a new western bar, previously unknown and of unstated provenience (11 November 1999).

[16] Thirdly, in the same introduction the Editor reaffirmed, for the first time in print, what had been decided earlier concerning Hodder, that now no reply to Bowers would be permitted either: "This communication will conclude the discussion on this topic in

any of the publications of the American Numismatic Society". In fact of course Hodder's *AJN* article appeared subsequently, it too asserting the authenticity of the bars.

This and the other instances gathered above illustrate clearly enough the collapse of scholarly integrity at the Society under its present administration. Nor is it accidental or off-hand; it chimes with what is now ANS announced policy, that the Society is only a forum and that its pages are open to any submission by anyone in a position to pressure it (except what its officers would not like to be made public).

Houghton, Kroll and Wartenburg have contributed directly, and knowingly, to the corruption of our studies. In my judgement they are unfit to hold any position of trust in a learned society (or in a university for that matter). So too those members of Council who knuckled under to the dealers and agreed to the suppression of further discussion of a major problem in American numismatics. It may be pertinent that both the current President of the Society, Donald Partrick, and a recently elected member of Council, John Adams, are personal friends of John Ford.

This whole affair is so outrageous and so indecent as to raise the most serious questions about the scholarly commitment and future credibility not just of the ANS, but of all of us who have individually contributed to it, and to our discipline, over the years. The Society has had a glorious scholarly history under others' leadership, and we have been part of that; it is now disastrously in the hands of people who have betrayed that tradition, who have quite a different agenda, people for whom protecting their own back is of more importance than intellectual integrity, or indeed ethical integrity.

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